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BEFORE THE
SUBCOMMITTEE ON ENERGY RESEARCH AND WATER RESOURCES
SENATE COMMITTEE ON INTERIOR AND INSULAR AFFAIRS
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Mr. Chairman, I appreciate the opportunity to testify on the subject of the energy research and development and the Administration's National Energy Plan.

I am accompanied by my colleague Kendrick Wentzel, by Douglas M. Costle, Assistant Director of CBO for Natural Resources and Commerce, and by Nicolai Timenes, Jr., Deputy Assistant Director.

At the request of the House and Senate Budget Committees, the Congressional Budget Office (CBO) undertook an analysis of Energy Research, Development, and Demonstration strategies. This analysis is reported in a CBO background paper entitled, "Energy Research: Alternative Strategies for Development of New Energy Technologies and Their Implications for the Federal Budget". The paper has been published, and we are happy to provide copies to the committee for the record.

An earlier draft of that paper was provided to the staff of this committee, who made many comments which were helpful in our revisions. That paper is the basis for my testimony today.

Under its statuatory charter CBO is required to analyze policy options for allocating resources, but it does not make recommendations, and I will make none today.

I would like, however, to provide the committee with a perspective on the design of alternative strategies for research, development, and demonstration.

We want to establish a thread which will permit us to connect broad energy policy considerations with specific energy research decisions. To do this, let me pose a series of questions, the answers to which will help to illuminate the choices for energy R&D:

- What can and should the nation's energy future look like?
- What can and should be the role of new and emerging technologies in shaping that future?
- What are the impediments to the development and timely introduction of the desired new technologies?
- What, then, should be the criteria for design of a federal research strategy?

I would like, today, to discuss those questions--not to answer them--and to suggest several alternative research strategies which might flow from different answers to those questions. Finally, I will discuss the budgeting implications of the alternative strategies.

ERDA's national plan is a start in the process of designing a research strategy. A complete strategy is at least implied by the combination of that plan with ERDA's program implementation, with the President's budget request for fiscal year 1977, and with the plans and program implementation for the years ahead, as proposed by the President and modified by the Congress.

What Can and Should the Nation's Energy Future Look Like?

During the last few years, a consensus has emerged about certain objectives of a desirable energy future. Those objectives include sharply reduced reliance on imports, improved environmental quality, and a degree of protection of the economy against the effects of rapid large increases in energy prices. The consensus extends to general specification of the methods which could be used to achieve these objectives. Such methods include wise use of available energy (conservation), efforts to accelerate development of domestic resources, development of renewable resources or of those which are essentially inexhaustible, and an expanded role for new technologies.

The consensus does not yet extend to a specific balance among objectives or among means of achieving them. Indeed, much of the current energy debate focuses on such issues. In its national plan, ERDA presents several alternative scenarios for the future ranging from no change in current trends through cases involving heavy dependence on conservation or on nuclear power to one requiring contributions from all energy technologies.

What Can and Should be the Role of New and Emerging Technologies in Shaping that Future?

Again, there is general consensus that technology, and particularly new energy technologies, will play an increasingly important role in such a future. To use ERDA's phrase, new technologies can create options for the nation's energy future. Much has been made of the potential qualitative and quantitative contribution of energy technologies that are now only on the drawing board or in the laboratory.

What are the Impediments to the Development and Timely Introduction of the Desired New Technologies?

The impediments - technical, environmental, economic, or institutional - to implementation of new or expanded technologies, generate requirements for research.

Some of these are due to uncertainties about technical feasibility, about environmental impact, and about prices. Some of the impediments are not due to uncertainty: the costs may be known to be too high for profitable operation, or perhaps environmental standards could not be met. An R, D&D strategy can be designed to gather information to reduce uncertainty. The decision then must be made whether to try to pursue the implementation or not. In some cases a strategy can help overcome impediments other than uncertainty, but in other cases different mechanisms such as incentives may be more appropriate.

What, Then, Should Be the Criteria for Design of a Federal Research Strategy?

There is some consensus that federal support for research is justified when the payoff of that research for society as a whole may be greater than that which could be realized by an individual entrepreneur.

In addition, it is sometimes argued that the social benefits of advancing new technologies--whether they be reflected in increased safety, lower environmental impact, or reduced oil imports--justify federal expenditures beyond those required simply to prove that a process will work.

Given these perceptions of the future and of the role of technologies in that future, can general criteria be suggested for the design of a research strategy?

The Nonnuclear Energy Research and Development Act of 1974 sets forth several criteria for the inclusion of individual projects in a federal program. For simplicity of presentation today, I would like to suggest that criteria for design of a research strategy can conveniently be divided into three broad categories:

(1) support of desirable futures,

(2) insurance against failure, and

(3) cost.

The degree to which a given research strategy supports desirable futures depends on its consistency with Congressional mandates emphasizing energy conservation, renewable resources, and environmental technologies; its support for an energy future not limited to a few sources but drawing from a wide

diversity of types; and the balance it achieves among technologies that could provide energy over three time frames: near-, mid-, and long-term.

Providing insurance against failure minimizes the chance that all or many technologies will prove unfeasible, uneconomic, or impractical (as some surely must). The degree to which a strategy satisfies this criterion depends on: its adequacy of attention to basic (as distinct from applied) research; the pursuit of a diversity of technical approaches to any one source, (for example supporting both centralized and localized conversion of solar energy); balance in the scale of research so that large costly demonstration projects do not crowd out earlier stages; and proper pacing, so that the program does not proceed so quickly that problems and failures raised in one stage are incorporated into the next.

Finally, cost is an important criterion because federal support for an endeavor, no matter how laudable, is not unlimited. Our projections of the costs of energy research suggest that they will increase substantially over the next decade, making it extremely important to choose wisely, especially with respect to large, expensive projects.

Alternative Research Strategies

The considerations about the future and criteria do not unambigously determine the best research strategy. Several other questions must be asked:

- What overall level of funding for energy R, D&D should be supported?
- What is the appropriate mix of nuclear fission versus nonfission technologies?
- How much effort should go to large demonstrations versus smaller-scale research and development?
- When should specific decisions be made?

In order to illustrate answers to these questions, we have designed five alternative research strategies, which differ in the degree to which they satisfy the above criteria and which provide different answers to those questions.

These five strategies are, in order of increasing budget impact:

(1) A strategy continuing present programs, completing ongoing projects and allowing for modest real growth, but not allowing for major new starts. This we call a "base program completion" strategy.

(2) A strategy emphasizing near- and mid-term technologies, and deferring all major long-term technology projects not already underway.

(3) A strategy emphasizing long-term nuclear fission technologies and down-playing all others.

(4) A strategy down-playing the fission programs but emphasizing all other long-term technologies.

(5) A full funding strategy, including the first strategy and carrying out all major energy R, D&D projects identified by ERDA in its national plan.

Budget estimates for these five alternatives have been developed by combining the President's budget request for fiscal year 1977 with the elements of the national plan proposed by ERDA. Additional assumptions had to be made about the real growth of programs and about the costs of potential large-scale demonstration projects identified in the ERDA program implementation document.

The base program completion strategy uses the President's fiscal '77 budget with its five year projections as a base. Modest growth was added and completion of projects already begun was assumed. However no new projects would be initiated beyond 1977; it is very close to a "no new starts" strategy.

This strategy would explicitly allow present program trends to continue with few major new processes developed at the demonstration scale. This reliance upon old projects and priorities seriously reduces its consistency with desirable futures, since it would not respond to new priorities in solar energy, conservation or environmental protection, or in diversity of new sources. While the pace of development would not be excessive, the lack of diversity is likely to raise risks of failure in the future.

The strategy emphasizing near and mid term technologies would include all components of the base program completion strategy but would add those projects identified in the national plan whose results could possibly be implemented in the near or mid term.

This strategy would not be completely consistent with desired futures, because long term demonstrations would be deferred. Since long-term processess would be supported at less than the demonstration level, there would still exist some diversity of sources, including conservation and renewable resources, helping insure against failure. On the other hand, if lead times are critical, some long-term technologies could be delayed.

If, instead of emphasizing near-term technologies, support of long-term solutions is deemed desirable, then two alternatives suggest themselves. The first of these, emphasizing nuclear fission technologies (especially the breeder), would add to the base program completion strategy all demonstrations in the nuclear fission programs, but would not include demonstrations of other technologies.

This neglect of important nonfission sources, conservation and the environment, would not be consistent with ERDA's desired future. It might not insure against failure by focusing on only one technology even though all time frames are covered. Pacing might also be a problem, because of heavy emphasis on a single technology.

An alternative long-term strategy, that emphasizing nonfission technologies, would add to the base program completion all the demonstrations for nonfission areas but would not include new demonstrations for nuclear fission.

This strategy would also be inconsistent with ERDA's desired future since it does not assign an important role to the breeder. On the other hand, support of a wide diversity of sources other than the breeder may help insure against failure.

Finally, the full funding strategy would add to the base completion strategy all of the demonstration projects identified in ERDA's national plan in all program areas.

This strategy would be consistent with ERDA's desired future in that all program areas would be supported, providing

diversity in source and time frame. Its weakness stems from the scale of effort and the pace. So much of the effort is in demonstration that budget pressures could imperil initiatives in areas not yet programmed.

Budget Impacts

The total costs over the next decade for these strategies range from over $40 billion for completion of the base program to nearly $64 billion for the full funding strategy. The budget impacts of each strategy are summarized in Table I, which is included on page 11 of my testimony.

Each of the strategies I have discussed implies future authorizations significantly higher than current levels. A strategy whose budget would be lower than current levels is certainly possible. Such a strategy, however, would require a major shift in Congressional priorities.

Figure 1, which appears on page 12 of my prepared testimony, shows the budget authority required for each strategy over the next ten years. With the exception of the base program completion strategy, which increases slowly, the budget authority required for each strategy rises to a maximum and then declines, due to the fact that projects planned now can extend a research program only so far. Actual budgets after 1980 would depend on intervening decisions about budget levels and on the desirability of planned projects, and on budget requirements for introduction of new projects.

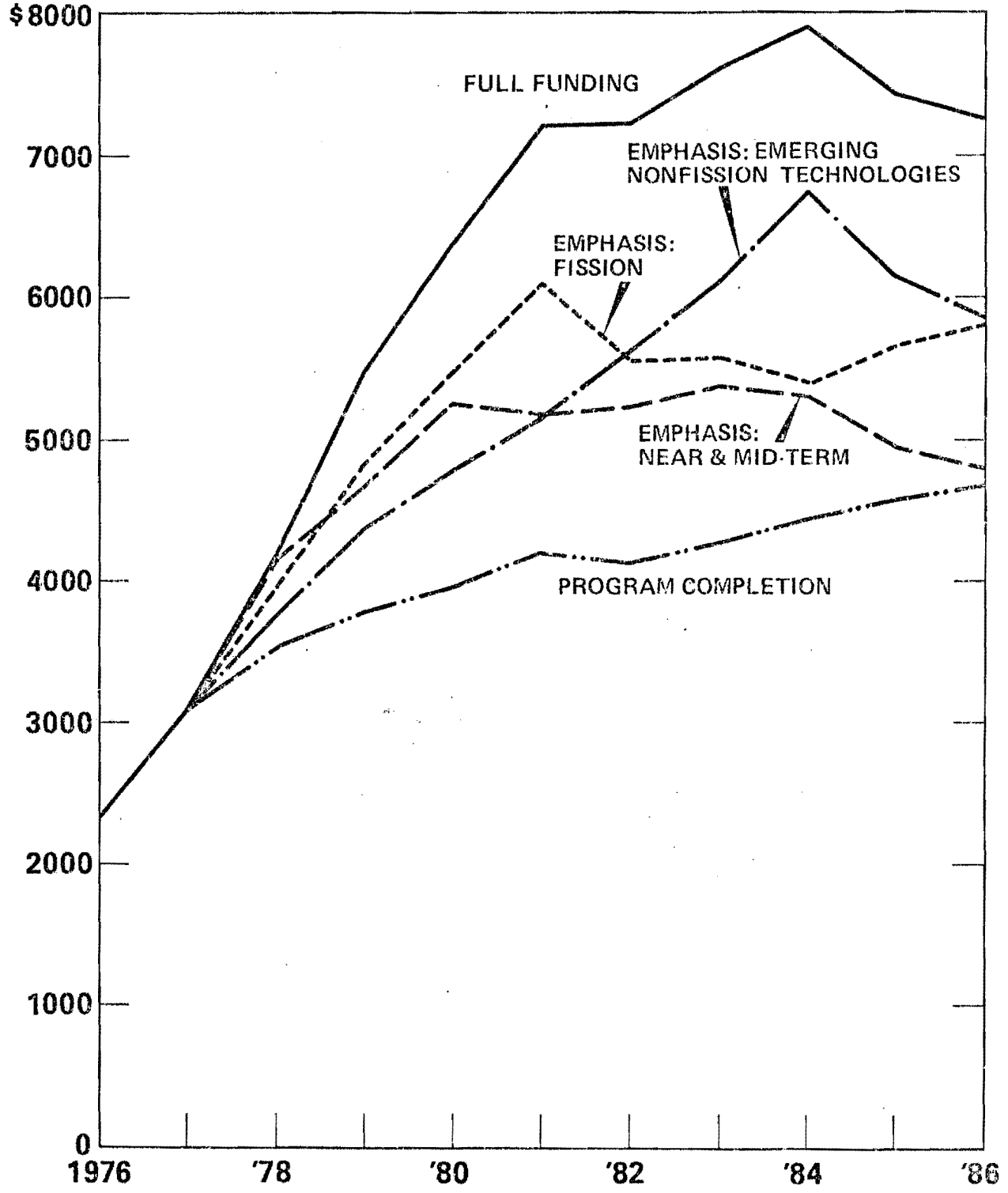
TABLE I

FIVE ENERGY R,D&D BUDGET OPTIONS
(Billions of 1977 Dollars)

Strategy	Cumulative Ten-Year Budget Auth.	Peak One-Year Budget Auth.	Year of Peak
Base Program Completion	40.5	4.7	1986
Near- and Mid-term Emphasis	48.0	5.4	1983
Fission Emphasis	51.3	6.1	1981
Non-Fission Emphasis	51.6	6.7	1984
Full Funding	63.7	7.9	1984

Chart 1 Budget Authority for Alternative Research Strategies, 1976-1986

(In Millions of 1977 Dollars)



Selection Among Strategies

If funding constraints are extremely severe, or if it is decided that future large-scale demonstrations should be the responsibility of the private sector, then it would be appropriate to select a strategy such as the base program completion strategy.

If, at the other extreme, the Congress decides that extensive federal support of demonstrations is appropriate, and that an additional $23 billion, beyond the costs of completing the base program, can be made available over the next ten years for energy research, then a full funding strategy could be selected.

Selection of one of the three intermediate strategies, which have remarkably similar budget implications, could result from a decision that some intermediate level of funding could be supported. Such a selection could also reflect a Congressional decision that, for funding or other reasons, only one of the three major groups of demonstration initiatives--near-term, fission, or nonfission technologies--deserves emphasis.

Other Perspectives on Research Strategies

The five strategies imply quite different mixes of fission and nonfission research over the decade. The range is from about $25 billion, or nearly half of all funding for fission in the strategy emphasizing that technology to about $13 billion, or only a quarter of all funding, in the strategy emphasizing nonfission technologies.

The influence of demonstration is also quite significant in the choice of strategies. As the support of demonstration becomes larger the inertia of a program becomes stronger. The full funding strategy would devote about 45 percent of all funding over the next decade to demonstration projects, while the strategies emphasizing fission, nonfission or near and mid term would spend about one-fourth and the base program completion about one-tenth on demonstrations.

Finally, I would like to point out that final, detailed decisions on a research strategy need not be made now. Perhpas they should not. The budget paths which we have illustrated would result from a series of decisions to be made over the next decade. Research is dynamic, and new information becomes available almost daily. That information is useful by itself; it can also be used to help shape those decisions to be made in the future.

But, each decision represents a new commitment. The pattern of those decisions over the next few years--especially with respect to major demonstrations will--whether by conscious design or by piecemeal actions--result in the definition of a strategy. As we have seen, that pattern of decisions will have important impacts on the federal budget over the next decade.

This concludes my formal remarks. My colleagues and I will be delighted to answer any questions you may have.